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MATHEMATICS IN NINETEENTH CENTURY SCIENCE[1]

THE treasures of one age are the rubbish of the next age. Ideas, like things material, are mostly transient. The present possesses but little of that which the past, with infinite labor, has acquired. Our estimate of values changes from century to century, and often with reason: what was once useful is found under later conditions to be wasteful, and new knowledge piles old machinery upon the scrap-heap.

Considered in this light, the science of even one hundred years ago looks antiquated to a schoolboy of to-day. But what of the exceptions? Not all knowledge is novel, and there are indispensable truths and fundamental principles that were discovered thousands of years ago. Most of our exact science is, however, new since the time of Galileo, Bacon and Newton; and it is probably not far from the truth to say that three fourths of the knowledge at present constituting exact science was discovered in the course of the nineteenth century.

Every generation must either advance, or lose much of what it has inherited; only as it is used for finding new knowledge is the value of the old science understood. I speak to-night to a group of younger students of science, into whose hands are committed from the past whatever they can use of accumulated knowledge; and who have announced, by the badge of Sigma Xi, their devotion to the highest ideal in science, that of increasing its definite content

[1] Address before the Syracuse Chapter of the Sigma Xi, March 15, 1915.

and improving its applications for the welfare of all men.

Every man must do what he can—hence comes specialization. Mathematics is and has been a useful kind of work, of value both immediate and prospective. It is of that I am to speak here briefly. On its utility I need touch but lightly, mentioning a few of the most obvious contributions to other branches of science; then I must point out more at length certain particular mathematical theories and bodies of reasoned abstract knowledge developed in the past hundred years.

These latter take their chance of survival along with the poetry, the art, the philosophy of their time—and indeed with much of what is now received as natural science. If it survives, it will be because you, and others like you who are to become intellectual leaders in the near future, find in these fields tasks which seem to you worth the doing; things begun which you would gladly finish, errors which must be cleared away to make room for truth; ideas in germ or questions vaguely hinted at, which can be worthily developed by your arduous labor.

First, then, let us recall some of those mathematicians whose labors have enriched other natural sciences since the time of Lagrange and Laplace. Four physical problems of major importance have demanded the devotion of mathematicians of the first rank, and have given occasion for the elaboration of theories now generally accepted. These are the problems of the transmission of light, of electrical and magnetic effects at a distance, of the relation between heat and other forms of energy in the world of force, and the historical question concerning the origin and growth of the earth on whose surface we live.

Not that the statement of a physical theory requires a mathematical mind; indeed the observatory and the laboratory are far more likely to be the birthplace of theories than the computing room or the logician's study. But whoever formulates a physical theory with precise terms, definitions and laws, and tests it for consistency of its parts and agreement with a wide range of facts—he is a mathematician; and if the complexity of his problem drives him to invent new concepts or new short-cuts in argument, he is a creative mathematician.

Such was Fresnel, who in 1817–19 analyzed and pushed to precise formulation the theory of wave-motion in the luminiferous ether. The hypothesis of an ether was not unknown at that time, and in acoustics the undulation theory was well established. Authorities, however, seemed overwhelmingly in favor of the emission theory of light—Descartes, Newton, Brewster, Laplace and Poisson. It required the resolute and unperturbed mind of a true investigator to give due attention to the hypothesis, then far from orthodox, of an all-pervading ether, and to build up a complete theory of the phenomena of diffraction until it brought him to a crucial experiment, which even his opponents admitted to be decisive. When its implications were completely analyzed and their consequences demonstrated, doubt and prejudice gave way to clearness and certainty. The controversy was practically closed when in 1820 Fresnel received the medal of the Paris Academy for his essay on "The Diffraction of Light." And this general indorsement of the ether hypothesis was most essential for the next pressing problem, that of the transmission of electrical effects.

The effect of a small closed current of electricity upon a magnetized particle in its field is like that of a magnet, feeble or strong, standing at right angles with the plane of the current. One closed current attracts or repels another, just as one

magnet acts upon another; and a current closed or broken in one circuit occasions a current in another closed circuit. Experimental studies of these phenomena by Faraday and Weber revealed quantitative laws, but seemed to show instantaneous effects—forces acting at a distance with no delay in time. The marvellous intuition of Faraday, not himself an analyst, but certainly a profound inventor of geometric motions, created an ideal structure of tubes of force, with something flowing through them under hydrodynamical laws. This bold concept served as basis for the calculations of three mathematical minds that took up his great problem. Sir William Thomson, later known as Lord Kelvin, Helmholtz and James Clerk-Maxwell, each in his own way set forth, in precise notations, equations describing the amount and direction of the transmitted forces. Thomson and Helmholtz ventured hypotheses upon the nature of the transmitting medium and its motions, culminating in those vortex-rings and vortex-sheets which were studied eagerly two decades ago.

A closed vortex-filament in a perfect fluid was shown to be indestructible, and ardent was the hope that properties and differences of vortices would be found analogous to those of the indestructible atoms of chemistry. But the third, Maxwell, penetrated in another direction, and showed what ought to be the rate of transmission of electrical impulses or waves, through an ether such as carries waves of light. The result, that electrical effects travel with the speed of light waves, shows logic outrunning even imagination. Hertz, almost the equal of these three as a mathematician, still greater as an experimenter, actually sent out and collected again such waves, a hundred thousand times longer than waves of light, reflecting and refracting them like light, and so confirmed the speculative conclusions of Clerk-Maxwell. In this exciting race to show the analogy, resemblance, or even identity of things apparently unlike, the study of vortex-rings was suffered to lapse. Or was it because physicists perceived that atoms were not so simple as had been supposed; that it would require, for the explanation of a single atom, more than one ring, however intricately self-involved? At any rate, there remains that one fragment of theory to be revalued and completed by some genius of a future generation.

Certain passages in the preface of Maxwell's "Electricity and Magnetism" show so clearly the relation of mathematical to experimental science that I can not refrain from extracting them verbatim; but first I will quote a general remark from Samuel Beidler upon accuracy.

> The appreciation of the value of accuracy is a thing of modern date only—a thing which we owe mainly to the chemical and mechanical sciences, wherein the inestimable difference between precision and inaccuracy became most speedily apparent.

Maxwell's idea of the way in which deductive methods come to be applied to phenomena is compressed into these two passages. Observe that measurement is fundamental.

> I propose to describe the most important of these phenomena [electromagnetic], to show how they may be subjected to measurement and to trace the mathematical connections of the quantities measured. Having thus obtained the data for a mathematical theory of electromagnetism, and having shown how this theory may be applied to the calculation of phenomena, I shall endeavor to place in as clear a light as I can the relations between the mathematical form of this theory and that of the fundamental science of dynamics, etc.
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> There are several treatises in which electrical and magnetic phenomena are described in a popular way. These, however, are not what is wanted by those who have been brought face to face with quantities to be measured, and whose minds do not rest satisfied with lecture-room experiments.

Though he insists that Faraday's methods were mathematical, merely expressed in symbols different from those usual among other mathematicians, yet the world knows that Faraday's labors could not have borne such abundant fruit, had there been no Maxwell to interpret and push to their limit his theories. By the combined labors of physicist and mathematician it was finally established that electro-magnetic action at a distance is due to disturbances of the same ether which conveys light-waves, and that this action occupies measurable time; that its velocity is indeed that of light itself, and that light-waves are of the same nature as those sent out from an electric current periodically interrupted. Compare this certainty with the state of doubt, at the beginning of the nineteenth century, upon the relative merits of the corpuscular theory and the undulation theory of light. Refined measurement and rigorous logic had indeed produced a visible effect!

In 1873 was published Maxwell's immortal treatise on "Electricity and Magnetism." In the same year appeared the first work of Josiah Willard Gibbs, then a young professor of mathematical physics in Yale College, on "Graphical Methods in the Thermodynamics of Fluids"; and only five years later his most important work, "On the Equilibrium of Heterogeneous Substances." By this time the great law of the conservation of energy was fully recognized, but its detailed implications were mostly still vague and imperfect. It was certain, however, that in each conversion of energy into other forms there was a degradation of a part: that not all the energy present could ever be utilized as mechanical force; some small percentage was always reserved in the form of heat, or electric potential, or chemical energy, or otherwise. Some vaguely understood quantity called entropy was in the field, so that the total of energy present was divided between entropy and free or available energy. Gibbs set himself the problem: Given all the masses and energies present, of every particular kind, in a physical event, to specify the amount of each kind that will be present when equilibrium is restored. In short, he wished to express as precisely measured quantities the facts implied in the conservation theory. As a corollary, he verifies the brilliant dictum of Clausius, that entropy is a continually increasing quantity.

There is evidence that Maxwell's work waited fifteen years for its full effect to be felt in the scientific world. Gibbs's researches and theories waited somewhat longer, but are now recognized quite generally (I quote his biographer) as being "among the greatest and most enduring monuments of the wonderful scientific activity of the nineteenth century." One may say in brief that Gibbs passed from known laws of physical and chemical action in infinitesimal regions, to reach the succession of transient conditions and to describe the limiting condition of equilibrium, toward which the total finite mass must tend—a maximum of entropy, a minimum of free energy. It is certainly plausible to say that as he dealt with infinite systems, varying from point to point as well as in time, he was forced to invent statistical methods and to rely upon the theory of probability. The most remarkable feature of his work was the fact, noted by his French and German translators and editors, that its theorems reached beyond truth as experimentally known, and served as guides for laboratory research. To paraphrase his biographer, the important and admirable thing in his work is not any new physical hypothesis, but the extraordinary mathematical power which deals simply and rigorously with relations of great apparent

complexity. It is not surprising therefore to find Gibbs almost equally distinguished in difficult fields of pure mathematics, the geometry of N dimensions and in vector algebra.

I have not yet mentioned astronomy, nor living scientists; but can not forbear to call your attention to the apparent decline and fall of the Laplacean theory of the earth's genesis from a nebula, its slow concentration and shrinkage. Two eminent scientists of the present day, Chamberlain and Moulton, have resolutely insisted upon precise formulation of hypotheses, have subjected them to calculation as exact as the case admits, and seem to have established the superior probability of their planetesimal hypothesis: that the major part at least of the earth's mass is the result of slow accretions from intercepted streams of meteorites. It may be that a new theory of nebular evolution must be constructed, starting from the spiral arrangement visible in so many of Hale's and Ritchie's photographs. Certainly there is a strong temptation for younger scientists to join in the working out of this great problem, now successfully past its initial stage.

Most scientists can and will become mathematicians when their special problems reach the stage where measurements are possible, and pure mathematicians should be eager to discuss concrete problems when they see the possibility of applying methods that they understand. But there is an independent territory of pure mathematics, a realm of the understanding and the reason. Its fields have been explored, subdued and cultivated by men of genius, men of strong imagination, men of patient diligence, and by adventurers, ever since the beginnings of history. If the triumphs of natural science loom larger before the eyes of the average man, it is on account of his intellectual position and the distortions of perspective. Has man yet included in his scientific knowledge such a part of the now knowable universe as would be represented by any finite fraction, however small? Is all that the whole race has known, compared with the secrets yet to be discovered, as considerable as the smallest twinkling star among the fiery millions of the galaxy? Are not all scientists, with Sir Isaac Newton, children wandering beside the sea and gathering the pebbles that please them? The intellectual booty gathered by pure mathematicians in the past century was relatively not less magnificent than the fragments of understanding secured by scientists of the concrete; nor is the one kind, in the last analysis, more purely intellectual than the other. All true science is rational, and belongs equally to the reason. I shall name but few of the nineteenth century discoveries in pure mathematics, and not, perhaps, the most important; for in the record of times so recent each will necessarily praise the things that he himself has most admired.

Geometry has made more numerous and more important advances than in the previous three centuries. Her devotees have been numbered by the hundreds. Read the appreciative chronicles of Professor Gino Loria in "Il passato ed il presente delle principali teorie geometriche," in which he sets forth a noteworthy thesis, first propounded by the great French geometrician, Chasles. This science, he declares, is the most attractive, because the humblest worker may hope by diligence not merely to survey the edifice, but to build it further. Genius is no longer indispensable to him who would add a stone to its walls. This exhortation is equally valid to-day; and for this reason a young mathematician may well devote some time to geometry, even if his ultimate dream leads elsewhere.

The earliest years of the century saw the

rise of a geometry freed from Euclid's postulate of parallels. The chains forged by habit and by authority were broken, and with ease when once they had dared the attempt, men found that the existence of one parallel to a line through a given point was not the only workable hypothesis: all other axioms and postulates of Euclid might stand, while instead of this one they substituted either *no parallel*, or *more than one*. Lobachewski, Bolyai, Saccheri, and probably the great Gauss, were leaders in this memorable emancipation. It was left for the later decades to reflect upon the reasons and to furnish illustrations of the various possible kinds of systems of points, lines and surfaces. Along with parallels, of course right angles, the measures of all angles and the measurement of distances were subjected to revision; and late in the century Cayley and Klein invented the theory of projective measurement of linear segments and of angles, to re-combine the divergent kinds of systems into one harmonious theory. It is easy to misunderstand. I do not mean to say that non-Euclidean geometries require substantiation or sanction from the older system of Euclid; or that the kind of space which they describe presupposes a Euclidean space within which it may exist. The question of the true nature of the space we live in is equally foreign to all pure geometries. But our common experience accords sufficiently with the description given by Euclid, and men will always, no doubt, find his axioms preferable. Hence it was and always will be advantageous for us to have as illustrations of non-Euclidean geometries pictures of definite portions of Euclidean space and of objects therein which fit the described relations of other systems in other kinds of space.

Fortunately for my present theme, and its secular limitation, the end of the century brought a full and satisfactory discussion of the fundamental postulates of geometry, by Hilbert of Göttingen. This gave us a model for the examination of not only the traditional Euclidean and the two traditional divergent non-Euclidean geometries, but also for the testing of any other proposed system of fundamental postulates. For the first time, the consistency and independence of sets of axioms were tried and proven. And this was a boon equally to teachers of all grades; for the redundancy of text-book definitions and axioms in geometry had become an intolerable incubus to teachers of critical classes, who yet had not the patience nor the time for finding the solution of their own difficulties. Kant lived and philosophized too early. Axioms must now be judged by their utility for the purpose intended. But whatever they have lost in sacrosanctity and authority, far more is gained in freedom and in power.

Chronologically it is false, but in the inevitable logic of events it is true, that projective geometry developed simultaneously with non-Euclidean. The latter clung to measures but looked at parallels differently, the former viewed distance as changeable and considered parallels as intersecting. Descriptive geometry, the body of rules and relations collected in orthogonal projection, parallel projection, and central projection, acted as a stimulus or challenge. Here were a set of observed phenomena, partly reasoned, ready for precise definition and logical arrangement. On the other hand were visible the beginnings of algebraic geometry, presenting general methods and highly general theorems, threatening to engulf and obliterate all pure geometry except the most elementary. Let any student of analytic geometry reflect on how few theorems from elementary geometry the whole analytic superstructure rests! No wonder that those who preferred things rather than

symbols seized the most obvious means for enlarging the scope and abbreviating the processes of their favorite science! Into the existing knowledge they brought order and system, circumstances of the time gave it rapid development, and a new branch of science came into being.

So projective geometry was cultivated. It was the avowed rival of algebraic geometry. The problems solved and new theories advanced by Monge, Poncelet and Steiner were matched by the genius of Plücker, Moebius, Cayley, Clifford, Cremona and Sylvester. The theorems of the one kind, resting on algebra, were perfectly general; those of the other, founded on intuition of real elements, were compelled to state exceptions. To escape this obstacle, Poncelet stated the postulate of continuity, a logical, almost magical bridge over the lacunæ. But in algebra, when real quantities failed, there were the imaginary quantities to fill the gap. What could pure geometry exhibit as justification or explanation of the continuity that she had postulated? It was a recluse professor in a provincial university, von Staudt, of Erlangen, who settled the matter once for all with a perfect analogy. As algebra defines imaginaries by real quadratic equations whose roots are not real, so, according to von Staudt, geometry defines two imaginary elements by two real pairs of elements. In certain relative positions these determine two real elements; otherwise they stand as a real representation of two imaginaries. This is genius: to define the required object by the very phenomenon which constitutes the demand. What is sauce for algebra is sauce for geometry, and the imaginary elements are since that time the secure possession of both.

What then were the conquests of algebraic geometry? The ancients had examined conics and conicoids, that is, circles, ellipses, spheres, ellipsoids, and those alluring surfaces, the paraboloids, all loci of the second order. Sir Isaac Newton had made a pioneer study of plane curves of the third order, a venture in which for more than a century only two had followed him, until Moebius and Pluecker, about 1835, resumed the attack. It would take many hours to name in most concise form the new features and new problems that arose from this study. Inflexional points, Steinerian correspondences, poloconics, harmonic polars, Cayleyan and Hessian covariant curves—these will serve to remind some of you of the multiple ramifications of inquiries that began on plane cubic curves. Others will recall the metrical properties of semicubical parabolas and cissoids. Of quartic curves, the next higher order, even more is to be said—or omitted; their 28 double tangents and 24 inflexional points and many seemingly elementary problems connected with them remain unfinished, as students in all lands can testify, among others not a few Americans who have given labor and time to them.

Progress is often along converging lines. While geometry advanced steadily in the algebraic direction, algebra was acquiring a new concept, that of a GROUP of operations. Any set of operations form a group, when two of them unite to form always a third in the same set; thus, uniform expansions and contractions of an object form a group, and in numbers all multiplications and divisions together form a group. Now a group of operations will change some things and leave others unchanged or invariant. In algebra, the group of linear substitutions was the first to attract attention, and between 1845 and 1865 the study of this group was the most conspicuous business of algebraists. Soon it was recognized that in geometry all projective transformations constitute a group, and that

this is precisely the same as the group of linear substitutions, if points in space are given in rectilinear coordinates. From this it was not a long stretch to the conjecture that the properties of objects unchanged by projection must be expressible in some way in terms of the invariants discovered by algebraists. To work out this thought demanded the ardor and mathematical ingenuity of a race of intellectual giants like Cayley and Sylvester, Aronhold in Berlin, Hermite, Clebsch and Brioschi. Through their toil a special calculus was developed, and some progress made toward answering the central question: What, under the projective group, are the different possible invariant properties of single algebraic loci, and what the chief invariant relations of two or more loci or systems of loci? Here then was established a definite standard, by which it could be judged whether geometry was a science, or only the ideal program of a science. The group of operations, the simplest objects to be considered, and the invariant relations of those objects under the group: these covered the content, at least of projective geometry.

Probably no climax of equal significance for pure mathematics has been reached since Newton and Leibnitz took the scattered fragments of a theory of limits and from them created the differential and integral calculus. It was in 1872 that Felix Klein published from the University of Erlangen a brief program, or formal address upon assuming a professorate. The title was: Comparative observations upon modern geometrical investigations, and its central thesis was in essence the formal definition, just now mentioned, of geometry. There are many sorts of geometry, but all are alike in this, that each studies its own peculiar group of transformations, and seeks to discover and classify the properties of objects which are invariant under all the transformations of its group. This was then verified by a survey of all kinds of geometry developed up to that epoch.

Of especial interest is of course our elementary geometry, the standard Euclidean. We know its objects; what is the GROUP that it studies? Klein answers: The absolute position in space may be changed, for that change no one can distinguish. An exchange of right for left, as in the space seen in a mirror, does not alter relations that we call geometric. Moreover all size is merely relative, hence uniform expansion or shrinkage in all directions is an operation of the group. Hence rigid motion including rotations, homogeneous expansions and reflections against a plane, those with their myriaform resultants constitute the group of ordinary geometry. I have mentioned the group of projective geometry; others are the geometry of circles and spheres, admitting to its group all operations of elementary geometry and in addition all reflections upon spherical mirrors; the two kinds of non-Euclidean geometry, the four-dimensional geometry of lines, inaugurated by Plücker, and the geometry of contact-transformations, defined and begun by Sophus Lie, of Norway. Many others can easily be noted and named, all fitting Klein's description in so far as they are developed, by any student of mechanics, hydrodynamics, optics or indeed any perfected theory in physics. As science tends to become deductive, and as geometry is the most complete type of a deductive science, and now since Klein's program elucidates the ideal or norm of geometry, so it may well arrest the attention and illuminate the procedure of every systematic scientific investigator.

It must have been this mode of conceiving the essence of geometry that was before his mind when Gino Loria, the historian of modern geometry, wrote the fol-

lowing passage in the epilogue of his famous book:

> The figures of geometry which once appeared rigid and motionless—as one might say, lifeless, acquired from the theory of transformations an unlooked for vitality, by virtue of which they were changed one into another, disclosing thus kinships before unknown and establishing relations which had been previously not even suspected.

This expresses well the esthetic feeling of a scientist who ponders upon the meaning of his work, and it contains a hint of the mystery of the fleeting fact and the truth which endures.

Within the century we see geometry coming to definite ideal statements of her foundations and her aspirations. Hilbert has described the one, Klein the other. No longer are we to see interminable debates concerning empirical warrant or intuitive warrant for the truths of this exalted science; though these debates may be profitable, they are not geometry. Mathematics begins when we are agreed upon premises. No longer is there to be the illusion of completeness, as if the problems could all be finished, their invariants determined and interpreted. The question for an investigator who considers a problem is now that of the miner who is prospecting for precious metals; he must ask himself: Have questions like this proved simple enough to be solved, and have the results proved interesting or useful? As for the range of choice, there are appallingly long lists of classes of geometries in the new "Encyklopädie der Mathematischen Wissenschaften." Some I have named: differential geometry is full of attractions; the study of twisted curves and of the systems of curves on surfaces is practically still in its beginnings, with vigorous workers calling for recruits; finite point-systems have claim to early consideration; manifolds in space of more than three dimensions will later assume increasing importance; recent procedures in analysis must some time be subjected to geometrical statement in the hope of simplification; and nothing could be imagined more exciting than the geometrical and kinematical speculations upon the configurations or constellations that the physicists call atoms and molecules.

My choice of topics has been apparently capricious, for there is matter of importance and intense interest in all directions. It is worth mentioning that no fewer than five living Americans have produced books on the theory of functions, or some great division of that subject. Elliptic functions and the vast subject of hyperelliptic and Abelian functions are temporarily less active, while differential equations, and their successors, integral equations, are in the forefront of progress. The theory of numbers in its modern form dates back only to 1800, and teems with marvels unforeseen. It is within fifty years that Lindemann succeeded in proving the number π transcendental—the ratio of circumference to diameter, and it was almost twenty years later that the base of Napierian logarithms, the number e, was put into the same category. The classification and discovery of transcendental numbers is still going on. Concerning the combinatory analysis with its store of theories yet incomplete, one does not need to bring news to Syracuse, nor coals to Newcastle. But I may mention the researches on point-sets, set in motion by Kantor at Halle, and refer you to the summary views and keen commentaries of Van Vleck, professor in the University of Wisconsin, in his recent address as retiring president of the American Mathematical Society.[2]

The close of the past century saw the extraordinary growth of scientific societies, and in particular of mathematical societies. Three I may instance, all less than thirty

[2] See SCIENCE, Vol. 39 (new series), pp. 113–124.

years old with active membership ranging from 600 to over 900, the American, the German, and the Italian in Palermo. Taken with other things, these are signs of a flourishing condition of scientific thought. Possibly the most striking proof of this, so far as mathematics is concerned, is found in the annual quantity of published research which more than doubled during the last thirty years of the century.

No one understands the group of transformations which we call the flight of time, yet it acts unceasingly upon all human possessions. Nor are its invariants known; nor yet can we determine what part of scientific energy is conserved and what part is entropy, or waste. It seems to us now that the few great lines of development that I have so briefly traced do show permanent tendencies of organized knowledge—that in these directions science will at least not retrograde while our civilization endures. Yet it is already evident that the last word has not been spoken in physics, and conceivably the time may come when the names of Helmholtz, Kirchoff, Maxwell and Hertz will be venerated as that of Archimedes now is—hardy pioneers indeed, but no longer in the vanguard. Let me make the trite remark, that the transformations of time work more slowly on the body of treasure that we call pure mathematics than they do upon the far greater and more rapidly growing pile of natural science. The reason is obvious; natural science deals with an infinite number of data, and can never apprehend them all; hence she makes hypotheses serve temporarily. Mathematics does the same, but perfects her products by the progressive exclusion of conflicting data; that is to say, by increasing precision of terms. The Pythagorean theorem concerning the sides of a right triangle will be true longer, in the very nature of things, than Sir George Darwin's magnificent theory of the tides. This which is from one point of view a reproach to pure mathematics, constitutes on the other hand one of its titles to immortality.

That the literature of our science is vast and complicated shows only how many are the things that men have wished to know. More numerous, with every advancing decade, are the questions pressing for solution. It will not be your lot, members of the Sigma Xi, to discover anything so simple, necessary and universally useful as the multiplication table, or the common theorems upon volumes and areas; but you may find something as useful to mankind as Napier's logarithms, which were new only three centuries ago; or some theory as beautiful and perfect as that of elliptic functions applied to plane cubic curves. You may contribute to the labor of other scholars something as helpful as the great "Encyclopaedie" of the mathematical sciences, now almost completed by the untiring labor and devotion of cooperating mathematicians in all lands, but chiefly by Germans. But in whatever large domain or narrow field you may elect to labor, I give you the cheering assurance that there are fruitful discoveries that can be made by every toiler; that to each one who has the *will to know,* will come those rare and golden moments when he shall shout in triumph, with the ancient truth-seeker Archimedes, *Eureka!*

Henry S. White

Vassar College

SEEING YOURSELF SING[1]

It is possible to make vibrations which produce a tone to the ear also produce a picture to the eye—a picture which reveals details of pitch faithfully and far more finely than the ear can hear, and which may, therefore, be

[1] A part of a paper read before the meeting of the National Music Teachers Association in Buffalo, New York, December, 1915.